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JOURNAL

OF THE

MASSACHUSETTS ASSOCIATION

OF

BOARDS OF HEALTH.

ISSUED QUARTERLY.

PUBLISHED BY
W. S. FRENCH, WEST NEWTON, MASS.

OFFICERS

OF THE

MASSACHUSETTS ASSOCIATION

OF

BOARDS OF HEALTH.

1891.

PRESIDENT,

HENRY P. WALCOTT, M.D., Chairman State Board of Health.

VICE-PRESIDENTS,

SAMUEL H. DURGIN, M.D., Chairman Board of Health, Boston, Mass.
SAMUEL W. ABBOTT, M.D., Secretary State Board of Health, Boston, Mass.

SECRETARY,

LEMUEL F. WOODWARD, M.D., Chairman Board of Health,
Worcester, Mass.

TREASURER,

JAMES B. FIELD, M.D., Chairman Board of Health, Lowell, Mass.

EXECUTIVE COMMITTEE,

President, 1st and 2d Vice-Presidents, Secretary and Treasurer, ex-officio.

(*For One Year.*)

CHARLES H. MORROW, M.D., Gloucester, Mass.; M. R. DONOVAN, M.D., Lynn, Mass.; GEORGE L. TOBEY, M.D., Lancaster, Mass.; GEORGE F. BABBITT, Boston, Mass.; PROF. LEONARD P. KINNICUTT, Worcester, Mass.

(*For Two Years.*)

J. E. CLARK, M.D., Medford, Mass.; W. H. CHAPIN, M.D., Springfield, Mass.; W. K. KNOWLES, M.D., Everett, Mass.; EDWARD A. SAWYER, M.D., Gardner, Mass.; WILLIAM S. FRENCH, Clerk Board of Health, West Newton, Mass.

COMMITTEE ON SCIENTIFIC PAPERS AND PUBLICATIONS,

Vice-President SAMUEL H. DURGIN, Chairman; DAVID E. BAKER, M.D., Newtonville, Mass.; ERNEST S. JACK, M.D., Melrose, Mass.

COMMITTEE ON LEGISLATION,

SAMUEL H. DURGIN, M.D., Boston, Mass.; J. ARTHUR GAGE, M.D., Lowell, Mass.; JAMES C. COFFEY, Worcester, Mass.; E. R. CUTLER, M.D., Waltham, Mass.; EDWARD A. SAWYER, M.D., Gardner, Mass.

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MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

ORGANIZED 1890.

Vol. I. April, 1891. No. 2.

This Association, as a Body, is not responsible for statements or opinions of any of its members.

ANNUAL MEETING

OF THE

MASSACHUSETTS ASSOCIATION OF BOARDS OF HEALTH.

Held at the Massachusetts Institute of Technology, Boston, Wednesday, Jan. 21, 1891.

THE meeting was called to order shortly after two o'clock P.M. by the President, Dr. H. P. Walcott, who called upon the Secretary to read the records of the last meeting. The records were then read, and there being no objection, the President declared them approved, and then said the next item of business was the election of officers for the ensuing year.

On motion of Mr. J. C. Coffey it was voted that the Chair appoint a committee of three to nominate a list of officers for the ensuing year, and the President accordingly appointed Mr. Coffey of Worcester, Dr. Roche of Clinton, and Dr. McCollom of Boston, as the nominating committee.

THE PRESIDENT — Pending the report of the committee, perhaps it would be well if Dr. Durgin, as representing the Committee of Arrangements, should make a statement in regard to the programme of the business exercises.

Dr. SAMUEL H. DURGIN — I regret very much being obliged to announce the fact that we are disappointed in not having Prof. Lyman read a paper to us on rabies this afternoon. He expected to do so, but had made a previous engagement which could not be broken, to address a branch of the Massachusetts Medical Society this after-

noon at the College of Veterinary Surgery. Dr. Ernst, however, who was to make remarks upon the paper, will be present about four o'clock, and will address the Association. I also regret to say that Prof. Woodbridge, of the Institute of Technology, whom we expected to make remarks on the paper on Ventilation, is also obliged to decline on account of pressure of work and other engagements, but I have no doubt the time will be filled by other speakers.

It also occurs to me to say to the members of the Association that it was fully expected that the publication of the doings of the last two meetings would be finished, and at least ready for distribution at this meeting, and we all feel disappointed that this can not be done; but there have been delays beyond the control of the gentlemen who took the contract to print the report. We could have had it to-day, but it would have been incomplete, and it was thought best to delay it for a few days in order to have it complete. It will be mailed to each member at his home within probably the next week, or ten days at the outside. After this there will be no such delay; there will be no reason why you should not expect to have it within a very few weeks after the meeting has been held.

I would also say that immediately on the adjournment of the meeting, there will be two cars in front of the building to take the members to the Parker House, where the dinner is to be served.

THE PRESIDENT — We will ask if the Treasurer is ready to make his report.

The Treasurer, Dr. James B. Field, then submitted his report as follows: —

ANNUAL REPORT OF THE TREASURER, 1890.

RECEIPTS.

Annual assessments, 83 members	$249.00

EXPENSES.

Envelopes and postage	$22.08
Printing catalogues	25.70
Printing notices, circulars and bills . . .	11.00
Stenographic report of Worcester meeting . .	15.00
Type-writing of Constitution, etc. . . .	5.00
Amount carried forward	$78.78

Amount brought forward	$78.78	
Dinners for invited guests at Worcester meeting, etc.	18.00	
Record and account books	2.50	
Express	.30	
Total expenses	$ 99.58	
Balance to 1891	149.42	
		$249.00

JAMES B. FIELD, M.D., *Treasurer.*

Examined and approved as correctly cast and properly vouched for.

JOSEPH E. CLARK, M.D., *Auditor.*

The President — The report of the Treasurer will be received and placed on file. The Secretary has certain communications to make from the Executive Committee, and I will ask him now to make them.

The Secretary — The following names are recommended by the Executive Committee for membership: —

J. Arthur Gage, M.D., of Lowell.
W. P. Lawler, of Lowell.
Austin Peters, M.R.C.V.S., of Boston.
John F. McCartney, of Worcester.

The gentlemen named above were duly elected members of the Association.

The Secretary — The Executive Committee also recommend that a Committee on Legislation be appointed from the Association, to perform such duties as may be in their province.

The President — You have heard the proposition of the Executive Committee. Gentlemen, what action will you take thereon? Is there anything to be said on the subject?

Dr. Durgin — I move, Mr. Chairman, that this committee consist of five members of the Association, to be appointed by the Chair. (Seconded.)

Dr. E. A. Sawyer — Mr. Chairman, would it not be better, in order that other members of the Association may understand more clearly what the proposition concerning the Legislative Committee means as we discuss it, to explain what the purpose of the committee is?

THE PRESIDENT — I will ask the Secretary to explain it to the Association.

THE SECRETARY — The idea as talked up by some of the members, was that we should have a committee of the Association all ready on the ground to be on the look-out for any sanitary measure that may come before the Legislature, or before the Committee on Public Health, which committee should be the voice of the Association in recommending any sanitary project before the Legislature, and should appear before the Committee on Public Health in case of discussion as the voice of our Association.

Dr. SAWYER — My idea was that such a committee could announce to the Association when any important measures were to be acted upon, so that all would have knowledge of what was under consideration.

THE PRESIDENT — That was my own understanding. I would like to add to what has been said by the two gentlemen who have spoken on the matter, that this committee is simply to be the hand of this Association, and not the head of it.

(The motion of Dr. Durgin was then adopted, and the President said he would announce the committee later on.)

Mr. COFFEY — The committee appointed to nominate officers for the ensuing year beg leave to report that they recommend the re-election of all the present board of officers, with the exception of Dr. Griffin, of Malden, who, we understand, is not a member of the Association, and recommend as a substitute for Dr. Griffin, Mr. George F. Babbitt, of Boston.

Mr. W. S. FRENCH — Mr. President: I would like to ask what name the committee will substitute in place of that of Mr. Harlow, of Plymouth, who is also not a member?

THE PRESIDENT — I will ask if that difficulty presented itself to the members of the committee?

Mr. COFFEY — Do I understand the gentleman to inquire whom we will substitute? I was not aware that Mr. Harlow was not a member.

(The name of Dr. Peters was suggested, but he immediately declined.)

Mr. COFFEY — It is evident, Mr. Chairman, that there is no scramble for office here. The committee are ready to put in any name that is suggested.

THE SECRETARY — I will suggest the name of Prof. L. P. Kinnicutt, of Worcester. (Seconded.)

Mr. COFFEY — The committee will accept that, Mr. Chairman.

On motion of Dr. McCollom the Secretary was then instructed to cast one ballot for the list as amended, which he did, and the following were elected for the ensuing year: —

OFFICERS OF THE ASSOCIATION.

President	H. P. WALCOTT, M.D.
First Vice-President . .	SAMUEL H. DURGIN, M.D.
Second Vice-President .	SAMUEL W. ABBOTT, M.D.
Secretary	LEMUEL F. WOODWARD, M.D.
Treasurer	JAMES B. FIELD, M.D.

Executive Committee, for two years:

Dr. C. H. MORROW, of Gloucester.
Mr. GEORGE F. BABBITT, of Boston.
Dr. M. R. DONOVAN, of Lynn.
Dr. G. L. TOBEY, of Clinton.
Prof. L. P. KINNICUTT, of Worcester.

Executive Committee, for one year:

Dr. J. E. CLARK, of Medford.
Dr. W. H. CHAPIN, of Springfield.
Dr. W. K. KNOWLES, of Everett.
Dr. E. A. SAWYER, of Gardner.
Mr. W. S. FRENCH, of Newton.

THE PRESIDENT — There has been a provision made, as you will notice in the programme of this meeting, for the discussion of any general topics of interest to this Association, and perhaps now is a good time to introduce it.

Dr. J. A. GAGE (of Lowell) — I would like to bring before the Association a question in regard to a matter of legislation which might be referred to the new committee. In dealing with adulterated milk, we have a very good law, and in case of a violation of it, we have very little trouble in obtaining a conviction; but when we come to deal with provisions and meat, we find if we detect a man offering for sale articles of food which are improper, when he appears before

the court he pleads that he did not know this food was in an improper condition for sale, and he is usually acquitted; you cannot convict him. Well, that has this effect. The man says: "If I am detected by the Board of Health in selling tainted meat, I can go before the court and be acquitted by simply claiming I didn't know that this was tainted meat." He throws the burden of proof on them, and usually gets off, and says: "I will wait until the Board of Health come and seize the meat and destroy it for me, and that saves my doing it, and I will let them throw it away for me." The result is, the whole law becomes negative and inoperative; and I would suggest as a matter worthy of consideration by the Committee on Legislation a resolution striking out that word "knowingly," and I move that the Committee on Legislation present the matter to the Legislature to see if something can not be done to enforce these laws.

THE PRESIDENT — There are certainly present one or two gentlemen who have had a good deal of useful experience in these matters, and I am very certain that either Dr. Durgin or Dr. Abbott can tell us a good deal about the effect of such legislation as we now possess.

Dr. S. W. ABBOTT — There has been a law against the adulteration of milk for some twenty years, which at first was not very effective, but a change was made in 1880, since which time convictions have been made very easy, and the law has been undoubtedly productive of a great deal of good in the protection of the people. I do not know why some law should not be passed in regard to meat and other products.

Dr. DURGIN — I can fully endorse all that Dr. Gage has said on this question. I have had experience in the matter before the courts, and should say that this word "knowingly" is a serious stumbling-block in the way of clearing the markets of tainted meat. There is also something needed in the law pertaining to the sale of immature veal. As the law stands to-day, you might, upon evidence, convict a man and fine him for killing a calf under four weeks of age, but that is a very difficult matter, however, to get proof of; but we find it in the market for sale. All that you can do to-day, under the law, with the so-called bob-veal is to pick it up and carry it away, and that is very little satisfaction to the inspector of provisions or the officer of the law. What he needs to do is to pick up the seller instead of the veal, and take him to court and punish him as a man would be punished for any other crime. You cannot do it to-day.

I would say in regard to this word "knowingly," that many years ago we found that as applied to cases of adulterated food of all kinds, some sixteen or seventeen years ago, when the Boston Board prosecuted some cases of poisoned candy, every one who could not be proved to have manufactured the goods got clear by simply saying that he did not manufacture it, and to convict him it was necessary to prove that he knowingly sold it as adulterated candy or confectionery. Of course you cannot convict a man for knowingly selling adulterated goods, poisoned goods, or tainted meat. No man is going upon the stand and convict himself. A change in that law was then sought, and it was a very easy matter to get the word "knowingly" struck out, and that in itself cleared the market of Boston of poisoned confectionery. Not a single case has been found on chemical examination in Boston, to my knowledge, since, although it has been searched for; and so it will be with the tainted meat and bob-veal. When it is understood that a man takes a risk in attempting to sell this kind of material, he will stop selling. I am in full accord with the motion made by Dr. Gage.

The President — Is there anything more to be said on the subject? The question is on the motion submitted by Dr. Gage, that this whole matter of certain changes to be made in the laws governing the sale of tainted meats and improper food, be committed to your new Committee upon Legislation.

The motion was adopted.

The President — I will now announce your Committee on Legislation, the members of which were to be appointed by the Chair: Dr. S. H. Durgin of Boston, Dr. J. A. Gage of Lowell, Mr. James C. Coffey of Worcester, Dr. E. R. Cutler of Waltham, and Dr. E. A. Sawyer of Gardner. Is there anything else to be brought before the meeting, of general interest?

The Secretary — Mr. President: It was the intention of the Committee to allow a short time for the discussion of sanitary topics, and I hope members will avail themselves of everything they can think of. I want to bring before the meeting the subject of placarding houses in cases of contagious disease, and get the sense of the meeting as to the advisability of doing it. It is the custom in some cities to placard every house in every case of diphtheria and scarlet fever. In other cities those cases are handled without placarding. I should like very much to know the opinion of some of the members present as to the advisability of doing so.

Dr. W. H. CHAPIN — Mr. President: In Springfield we placard, as the Secretary said, all cases of scarlet fever. Up to about six months ago we had a bright scarlet placard six inches square. It was an offence to the people of the house placarded. We have now reduced the placard, and place it as near the door-bell as possible, not necessarily seen from the street, but so as to warn anybody going into the house that there is disease there; and that card is perfectly satisfactory, and I know the people would protest against discontinuing the practice. People come and want to know if a certain house has been placarded; it is in great favor among the people of the town. It certainly does a great amount of good in warning strangers not to go into such a house, and on the other hand, it attracts a number of people there who always run after such things. I am decidedly in favor of placarding houses with small placards put as near the door-bell as possible.

Dr. FIELD, of Lowell — We have placarded houses for three years, and would not discontinue the practice. The placards are as small as those mentioned by Dr. Chapin. The scarlet-fever card is printed in scarlet letters; that for diphtheria in black letters, on a light gray surface, supposed to resemble the color of diphtheritic membrane. When we came to measles we could find no color of card or ink, suggesting that of the eruption. The cards are placed on all doors of exit from the house, and remain on until the family physician reports that the patient is free from contagion, and the premises have been disinfected. Of course placarding the house does not enforce complete isolation of the patient, but only notifies the public of the existence of a case of contagious disease. My attention was recently called to a mild case of scarlet fever, where a careless mother allowed her child to play in the yard while still desquamating. Had there been no placards on the house, the neighbors would have allowed their children to play with the patient, not knowing that he was convalescing from scarlet fever. Poor people are glad to have these cards up, in order to keep neighbors' children out of the house. Our trouble is mostly with people who assume to know more than the physician. Membranous croup should be placarded, as being identical with diphtheria. Measles ought also to be placarded, for at times the mortality from measles and its complications is severe.

Dr. ABBOTT — Mr. Chairman: I would like to say in general on this subject, that I have examined the methods of many of our cities

and towns, and am very confident that this practice is one that has done a great deal of good. I know that the popular opinion differs much in regard to it, but I also know that it is gaining support more and more every year, and people expect something of the sort, if it is not done. I think in many of our cities and towns people begin to wonder why it has not been done before. Indeed, popular opinion has changed very much in late years. In some large manufacturing cities there are large foreign elements, and these placards have been printed in different languages, in the language of the quarter of the city in which it is, whether French or German, as the people may be of one nationality or the other, or in both languages. Isolation and quarantine have undoubtedly reduced the death-rate from these diseases. I do not think that is the whole cause of the reduction, but it certainly has gone a long way toward it. Look at the fact as to scarlet fever in the year 1889 as compared with previous years. The number of deaths from this cause in Massachusetts in 1889 was 185. In no previous year have they come down anywhere near that point, not even as low as 300, for thirty or forty years. Several years ago the deaths from scarlet fever went up as high as 1,500 and 2,000.* Now this is not a mere matter of the ups and downs of disease, as some

* *Mortality from Scarlet Fever in Massachusetts — 1856–1889.*

YEARS.	DEATHS.	Death-Rates per 10,000 Living.	YEARS.	DEATHS.	Death-Rates per 10,000 Living.
1856	1,252	10.9	1873	1,472	9.4
1857	2,013	17.2	1874	1,382	8.6
1858	1,051	8.8	1875	1,684	10.2
1859	1,038	8.6	1876	1,222	7.4
1860	916	7.4	1877	467	2.8
1861	1,137	9.2	1878	404	2.4
1862	1,261	10.1	1879	850	4.9
1863	1,399	11.1	1880	574	3.2
1864	1,503	11.9	1881	397	2.2
1865	807	6.4	1882	318	1.7
1866	385	2.9	1883	575	3.5
1867	828	6.2	1884	627	3.3
1868	1,369	9.1	1885	587	3.0
1869	1,405	9.9	1886	331	1.7
1870	683	4.7	1887	594	2.9
1871	867	5.8	1888	504	2.5
1872	1,377	8.9	1889	185	.85

people tell us (the anti-vaccinationists, for instance) ; much of it may be set down to placarding and isolation. Placards should be put upon all houses where there is small-pox, scarlet fever, diphtheria and the measles, and, if we ever should have it, yellow fever ; anywhere where there is danger of contagion. As to typhoid fever, it seems to me there is a question, where there is some objection to placarding, because it is not so directly contagious as these other diseases. It is infectious, but its liability of spreading is not so great as those diseases I have mentioned. The peculiarity of the infection in typhoid fever is such that the placarding of houses does not control its spread so much as the careful disinfection of excreta, and a watchful supervision of household water supplies.

Dr. J. H. McCollom.— Mr. President: In Boston cards are displayed for cases of scarlet fever and diphtheria, and I have no doubt that by this means the prevalence of these diseases has been somewhat diminished. The situation of the cards in apartment houses, of which there are a great many in Boston, is an important practical question. The suites are so entirely separate and distinct that a card on the entrance door of the flat in which the disease exists, is all that is necessary. When the Board of Health issued the order requiring the posting of cards there was at first considerable complaint, but now the public generally recognize the benefit to be derived from their use.

I quite agree with what Dr. Abbott said in regard to typhoid fever. The danger of contracting this disease by contact with the patient is so slight that it seems unnecessary to subject people to the annoyance of a card. So far as the posting of a card in cases of measles is concerned, I am of the opinion that, as there are so many mild cases of the disease floating about in the community, the mere posting of a card can have no appreciable effect in diminishing the number of cases.

One of the gentlemen who has just spoken says that it is the custom in the city which he represents for the family physician to remove the card. In Boston the card is removed by an agent of the Board of Health, after the premises have been properly disinfected.

Dr. Gage — Mr. Chairman: It would seem from my experience that some people go around seeking cases of measles that they can expose their children to ; but we believe in putting up cards in cases of measles, and I can illustrate how the practice can be of benefit by

stating how it has worked in my city. We sent our inspector to make a house-to-house inspection in all cases where children had been absent from school, and we found forty cases of measles which had never been reported to the Board of Health. The parents had not thought the children sick enough to have a physician, and the children were running loose in the street, if not actually going to school. I think it is the duty of the city to put a card on those houses. I agree with what has been said as to the contagion of measles, but I certainly feel we are right in putting on the card. In regard to the other point, we require a word from the physician before removing the card. We do not depend wholly upon it, but we believe the attending physician should have the right to say the child is well; but we make a rule that at least three weeks must elapse from the time of that child's sickness before we will receive any such notice. If a physician sends it any sooner than that, we will not receive it, except by our own investigation; but we recognize his authority in the case.

Dr. CLARK — Mr. Chairman: In Medford we use a yellow card having on it the words, "Scarlet Fever," "Diphtheria," etc., and when a case is convalescent the physician is required to report the fact to the Board of Health. An agent of the Board then instructs the parties how to fumigate their premises. The card is placed on the building by the police, and instructions are left as to such methods of prevention as the Board deems necessary in such cases. After the premises have been properly fumigated, the Board decides when a child shall attend school. In cases of unusual severity we have a quarantine card, warning people not to enter the premises without permission of the Board of Health. As far as we know, the people appreciate the warning cards. The school-teachers are informed of the existence of scarlet fever or diphtheria, and are instructed not to allow any children from an afflicted family to attend school until informed by the Board of Health that it is safe. In cases of measles the teachers are notified not to allow children to attend school where there has been exposure to infection. In case the Board is not informed, the teachers oftentimes know of existing cases, and assist materially by reporting the same to the Board. The same is true of the public library. We inform the librarian, and find by working in harmony, and taking these precautions, that it works to our advantage.

Dr. DURGIN — I will add one word to what Dr. McCollom has

said: — The schools in Boston are kept posted as to the location of known contagious diseases, so that they are enabled to shut out all pupils from those families. Every case reported is at once placed upon a list and sent to the schools, and it seems to me a very valuable means for preventing the spread of disease among the scholars.

Dr. KNOWLES — Mr. Chairman: In the town of Everett for two or three years we have placarded houses where contagious diseases were. At first there was some objection, but very soon we found that the people liked the idea very much, and if we neglected to have a card put on within twenty-four hours of the time we were notified, the people complained. We find it of great use, and we require the card to stay there until the patients are reported well and the premises fumigated. We used also to work with the truant officer for the schools. We reported any case of disease to him, and if he found any from such families attending school, he immediately reported to us, and in this way, of course, we were enabled to reduce these diseases very much, and I think it also had its effect on the public opinion, so that the people realized more the importance of taking precautions against the spread of these diseases. Placarding houses brought the matter before their attention, and I think we got very good results.

Mr. COFFEY — Mr. Chairman: One point the gentlemen do not seem thus far to have covered: why is it not possible that a great many of these cards may be torn down, and how difficult it must be to fasten upon the one who does it, and punish him for it. I would like to know from these men if they have any trouble of that kind in the various cities. Now, as perhaps you have surmised from what Dr. Woodward said, in Worcester we have not as yet placarded the houses. Our policy has been to immediately inform the school department in the morning, and the truant officers are there and immediately carry the news to the district in which the contagion exists, and the librarians of the three public libraries are also notified where contagion exists. We have had thus far excellent results from this method. We have done so well we are considering whether it is wise in addition to what we are already doing to placard the houses, and we have been doubtful as to the expediency of doing so, and I am glad to see that the consensus of opinion here is unanimously in favor of it. We have had very fair results in Worcester last year. Last year our deaths from diphtheria will show a reduction from the year before of

about ninety per cent. For instance, we had 38 in 1889, and last year 20 from diphtheria, and from typhoid fever 25, and last year 15; so we are not very badly off in Worcester; but if this method of placarding houses will increase our efficiency and further reduce the death-rate from those contagious diseases, the Board would be very glad to do that in addition to what they are already doing. We were very anxious to find out what were the various opinions of the gentlemen, and what their experience has shown to be best for the citizens at large; and I would like to know from some of these gentlemen whether they have had any trouble from cards being torn down.

Dr. FIELD — Mr. Chairman: I think our experience in Lowell is that not more than one or two sets of cards are torn down in a year, and then not by inmates of the house, but by some mischievous boys in the neighborhood. As a rule, people, from the highest class to the poor day-laborer, are very well satisfied to have the cards up until the danger passes. In regard to public-library books and public-school books when used interchangeably from one family to another, the custom has been this: to instruct our inspector when going to a house where contagious disease exists, to take away from that house all public-library books and public-school books.

Dr. DURGIN — To take the sense of the Association, I move that the practice of placing cards upon the doors of houses where contagious diseases exist be approved by this Association.

Dr. KNOWLES — I will second that motion; but I want to say in answer to Mr. Coffey's suggestion that our experience has been the same as in Lowell, as we have not had more than one per cent of the cards removed, and then probably by mischievous boys, as Dr. Field said, and we have always been promptly notified if they were torn down, so we have replaced them; and we make it a practice to have printed on the cards, which I presume is the general custom, "Any unauthorized person removing this card is liable to pay a fine of one hundred dollars."

THE PRESIDENT — The question is upon the resolution introduced by Dr. Durgin.

Dr. DURGIN — I would be very glad to amend my motion and make it that the Association approves placing cards upon houses, to remain until the premises have been fully disinfected by competent authority.

The motion as amended was seconded and adopted.

Dr. DURGIN — Mr. President: We have with us the Secretary of the Clinical Section of the Suffolk District Medical Society, Dr. Blodgett, who desires to present an invitation. I move that we give Dr. Blodgett an opportunity to speak.

THE PRESIDENT — The Association will be very glad, indeed, to hear from Dr. Blodgett.

Dr. BLODGETT — Mr. Chairman: I am very much obliged for your permission. I come, as representing the Clinical Section of the Suffolk Medical Society, to invite the members of this Association to attend the meeting of that body to-night, in which the topic for discussion is: Sewage; its Application to Land as the Best Method of Treatment; and knowing it is a subject in which the members of this Association must take a lively interest, from whom a proper understanding of that subject and its importance will reach people far more thoroughly than through the medical profession, I took the liberty to invite the members to attend our meeting at 19 Boylston Place, quarter before eight o'clock, and I can assure you, Mr. Chairman and gentlemen, you will all be welcome, and I hope it may be in your convenience for you to attend our meeting and take part in the discussion of that important question. [Applause.]

THE PRESIDENT — If there be no objection, the Association will now proceed with its regular programme, and I have the pleasure of presenting to you Mr. Frederic Tudor of Boston, who will be the first speaker of the day on Ventilation. [Applause.]

(For Mr. Tudor's paper, see page 72.)

THE PRESIDENT — Gentlemen: Dr. Ernst has very kindly, at considerable inconvenience to himself, agreed to say something to the Association upon the subject of Rabies, and if there is no objection I will ask that any remarks upon this question of ventilation or discussion of the subject may be postponed for the present, until after we hear from Dr. Ernst; and I, therefore, gentlemen, ask your attention to Dr. Ernst, who will address us as competent authority on the subject of Rabies. [Applause.]

(For Dr. Ernst's paper, see page 92.)

THE PRESIDENT — Dr. Peters is fortunately present, and we shall be very glad to hear from him what he has to say upon the subject. (Applause.)

(For Dr. Peters' paper, see page 102.)

Dr. DURGIN — Before we adjourn, I wish to move that the thanks

of this Association be extended to Mr. Tudor for his valuable paper on Ventilation, and also to Dr. Ernst for his valuable remarks.

(The motion was seconded.)

Mr. FRENCH — Mr. President: I hope the doctor will include Dr. Peters.

THE PRESIDENT — Dr. Peters is one of us, you remember.

Dr. DURGIN — Dr. Peters is a member of the Association, and he knows he has the thanks of the Association.

(The motion was then unanimously adopted.)

Dr. Durgin then extended a cordial invitation to all present to attend the dinner at the Parker House, and the meeting adjourned.

PROGRESS IN VENTILATING SCHOOL-HOUSES.

BY FREDERIC TUDOR, ENGINEER, OF BOSTON.

(*Read at the meeting held at Boston, Jan. 21, 1891.*)

It is characteristic of the human mind to seek an explanation of everything. The astronomer and the novel-reader, the chemist and the explorer of continents are all impelled by the same motive — curiosity. All wish to know what there is, where it is, how it is, and why it is. But it is only in recent times that the question has been asked, What is the cause of disease? It is not so long ago but that all who are present can remember it, that the inquirer was told that the cause of disease and premature death was the anger of the Creator; by their infliction the wicked were punished, the righteous chastened. Christianity has long cherished this belief, which is so strongly set forth in the Old Testament, especially in the Book of Jeremiah; nor have later priests quite given it up, though, fortunately for human progress, especially in the domain of hygiene, the generation is passing away which was content with this cruel faith.

The world is coming into the hands of another race, one which is in quest of truth and light, and, impressed with the belief that like causes assure like effects, is inclined to reject the supernatural and to rely upon facts yet to be discovered, though comprehensible, to reveal the explanation of every mystery. This is especially so in sanitary science; there is beginning to be a readiness to accept its discoveries and follow its teachings which promises great developments and is very encouraging to those who believe that the best way to promote health is to prevent disease.

Just now there is some interest in the sanitation of school-houses, which will certainly be greatly increased by proof of some actual progress. Interest and progress react upon and sustain each other; without evidence of success, interest will be discouraged and will abate. If nothing of real value is accomplished now, the whole thing may be dropped in disgust for another twenty years, as hap-

pened when an impetus was given to the subject by Franklin in the last century, and in this by the writings and efforts of Dr. Reid and our own Dr. Wyman, who still flourishes in Cambridge, and whose treatise on Ventilation, written many years ago, has long been regarded as a classic work. It would be well therefore to take account of stock, as it were, and see what we have to offer to an inquiring public in search of information. The time allowed limits us to a hasty review of the subject, treating only the leading points without much discussion or demonstration, and stating chiefly established facts and methods.

In the first place, what are the requirements of good ventilation? Before going in search of anything it is important to know exactly what is to be looked for. This rule has not always been observed, consequently some things have been found and applied which seemed very promising, yet were after all disappointing when made use of, because they were not exactly what was wanted; or, while they might have been a part, and a very important part, of it, yet were not the whole thing. Let me dwell upon this tendency to accept a part instead of the whole. It has been the chief cause of all the failures in practical work. These have been due not so much to error as to incompleteness. One swallow does not make a spring, nor does one law of nature, however carefully applied, make the art of ventilation; yet many schemes have been carried out by people who had acquired only one or two facts or knew only one or two laws, perhaps equally as wonderful and beautiful in their action as essential to any scheme, yet because less so than the many others which had been overlooked or neglected, they did not win success. A few years ago an expert in this mysterious art used to entrap victims by means of a well-known experiment with a glass bottle, with which I suppose many of you must be familiar. Placing the bottle upright, he lowered into it a lighted candle, whose burning speedily exhausted the oxygen to the point at which combustion is arrested. This happens, as you know, when only one-tenth of the oxygen is consumed. At this stage, when the flickering flame seemed on the verge of extinction, he slipped a strip of tin into the neck of the bottle, dividing it into two channels, through one of which fresh air poured in, driving the hot burned air out of the other and instantly reviving the almost extinct flame with a new supply of oxygen. The experiment is very pretty and instructive, and, as a flickering candle in a bottle bears some analogy to a breathing man in a house, especially when one considers that the

open mouth of the bottle is well represented by the chimney of the house, it was not very difficult to effect a sale of a patent ventilator to the delighted witness of the experiment. If a natural law would condescend to work with such promptness and certainty in the case of a common glass bottle, it would be hardly necessary to guarantee it to operate with equal certainty in ventilating the building where the patent ventilator should be applied. It is very evident that this is a phenomenon whereby a space containing hot spent air is replenished continuously with new fresh air, but it is not the whole art of ventilation, nor can it be applied except in extremely rare instances. The construction, use and arrangement of modern buildings are such that the process described cannot come into action at all. Yet the ventilators are still on sale, and are to be seen in every town in Massachusetts on buildings which deserve better treatment. So in regard to heating apparatus, it is some unduly magnified trifle or catch-penny device which decides the choice in most cases. Buyers are apt to prefer a plate steel furnace with a row of big handsome rivets round the top, which attract the eye and prevent it from detecting the open and loosely fastened joint round the bottom through which unlimited dust and carbonic dioxide are sure to pass directly into the fresh warmed air, and to reject the far preferable cast iron heater, with its extended, convoluted and gilled surfaces and with its apparently loosely fitted but practically gas-tight joints, well designed to withstand the effects of unequal expansion. A leading physician of Boston was chiefly instrumental in establishing the worthless and wasteful plate iron furnace in its present place of favor. He wrote a book based upon a mistake, and which is of course worse than a waste of paper, for it tends to perpetuate error. It is dangerous to speculate upon imaginary facts. Again, to show the undue influence of type or feature, how many times we hear it said that "hot water" is the right system to use, as if it carried with it everything that was needful and desirable in heating and ventilation. Good enough in itself, it is only a part of an apparatus from which a combined heating and ventilation may be obtained. It has been adopted for the most important building in Boston, the new Suffolk County Court House, where, instead of being subservient to the ventilation, the ventilation is made subservient to it — is but an incident of the heating. This is a case where, next to a hospital, the greatest sacrifices should be made for the sake of ventilation; everything else should

give way to it, architecture and all, yet the commissioners were so captivated by the merits and advantages of hot-water heating (which I, personally, do not concede, at least in this case,) that they quite overlooked every other detail. They accepted a part in lieu of the whole. When buyers don't really know what they want and turn to a salesman for advice, he is not very capable unless he can show them that he only has just what they need. If they have got plenty of money, so much the better for him; he has goods to suit all purses. This sort of thing is going on all the time; commissioners, boards of trustees and building committees, charged with the responsibility of getting the best for institutions in their care, are constantly adopting schemes of ventilation, because one or two things which they understand, or think they understand, seem to be plausible, but do not, because they cannot, give due consideration to other, perhaps more important, elements, to the great discouragement of competent and conscientious designers. Pardon this digression, but I could not otherwise show you the damage which is done by half-knowledge in this matter and by the disposition to overlook essential elements in the presence of one or two striking details. It is as if one bought a ship on the sole question of her ability to float, and neglected all the considerations of speed and sailing qualities in all winds and weathers.

At first thought it appears simple enough, this question of ventilation; it is only to find a means of allowing the close air to escape. It is only a question of opening a window where the cold draft will blow on some one else; or of making a hole in the ceiling to let the heat out; for most people do not distinguish between foul air and an uncomfortably high temperature. Today the average man believes that the open window or a hole in the ceiling is all that is necessary. With such an imperfect knowledge of the real demands of ventilation, really satisfactory means could scarcely be chosen. Fortunately scientific men have thoroughly investigated the laws of physics and hygiene which affect the question, and careful observers have accurately reported the results of well-considered experiments, in which they have shown how desirable conditions may be reproduced and what must be done to maintain them. The facts which they have collected form an immense fund which is accessible to every student, but whose volume is so great that I can here neither recount their steps nor repeat their demonstrations, but only their undisputed conclusions.

It is shown that efficient ventilation demands a continuous supply of a volume of fresh air equal to fifty cubic feet a minute to each adult occupant of an apartment, which implies the removal reciprocally of the same volume of vitiated air. It is shown (I am quoting de Chaumont, Pettenkofer, Angus Smith, Parkes) that although each man actually breathes only one-quarter of a cubic foot a minute, that this small volume combined with other bodily exhalations is sufficient to vitiate fifty cubic feet of the surrounding air to the extent of not being able to bear further vitiation without becoming noticeably tainted. The conditions are such that the supply of fresh air is to be poured into and become mingled with the already somewhat vitiated air contained in the room, and it is shown both demonstratively and experimentally that the vitiation can only be kept down to the limit imposed by considerations of health by the continuous supply of fifty cubic feet per minute to each occupant. It is obvious that if the air could be distributed to each person, a much smaller quantity would suffice, and that it could also be somewhat diminished in case the occupants are children. On the basis of fifty cubic feet to each adult man, thirty-three cubic feet would be the right quantity for the grammar school pupil, or two thousand cubic feet per hour, if the fresh air is pure and not already contaminated by being taken from a polluted source. Parkes says that the diminished consumption of air by young persons is balanced by the importance of favoring their healthy development by securing to them the best conditions, and advises that the standard of fifty cubic feet per minute per person be adhered to as the lowest limit. Moreover, the imperfections of apparatus must be allowed for. It is not practically possible to cause a perfect diffusion of the fresh air, thus preventing a disproportionate accumulation of vitiated air locally, nor to prevent some premature escape of the former by way of the ventilating outlets before its work is done. Experience shows that thirty, or even thirty-five, cubic feet a minute per occupant is not enough for school-rooms, and it is probable that the standard of fifty is too low unless the method of distribution is much improved over what prevails today.

It is a curious fact that the earlier theorists decided that five cubic feet per minute per person would be adequate ventilation. It is a very bad condition of things where scholars do not get more than this, and such cases are promptly condemned by the state inspectors. When Moody and Sankey's Boston Tabernacle was in use, it was

experimentally found by careful tests, the audience numbering about six thousand persons, that when the air supply was as low as five cubic feet per head per minute, fainting of a number of persons was certain to ensue, but that it was prevented altogether by increasing the supply to seven cubic feet. The fainting was confined to one locality remote from the fresh-air inlets; I am confident that with a better distribution than there was in some parts of this great hall, five cubic feet would have been adequate to prevent fainting altogether, if uniformly distributed to each person, although far from being adequate in the sense of proper ventilation. This fact is interesting because it shows that what was at first believed as a theoretical deduction would be sufficient for good ventilation, is proved by trial on a large scale to be the very least that will support life without distress. When this quantity was fixed upon, it appears to have been taken for granted that the five cubic feet of air would reach the individual for whom it was provided without any taint; in other words, the important element of distribution was overlooked. Later writers have taken this into account with a result showing that the quantity of the early investigators was ten times too small. This is a remarkable illustration of the uncertainty of purely theoretical deductions, or rather, of the importance of bringing every element into the question which may have any influence, and this cannot be done without thorough practical trial under all possible conditions. Weighed against a few facts obtained by actual trial, pages of discussion on paper are of no real value, except as mental gymnastics for mathematicians.

Accepting, then, the fifty feet per minute standard and applying it to the modern class-room of the graded school in the larger towns of this state, we find this is arranged for about fifty occupants; it has a space of about ten thousand cubic feet, or two hundred to each occupant. Great importance is attached to this matter of space, but it is apt to be overlooked that aside from questions of convenience of arrangement, it is the rate of ventilation, the time required for a complete change of the air, which determines the space. Thus, for a two-hours' session with no ventilation, the factors of the space are fifty cubic feet vitiated each minute by each one of fifty people, or $50 \times 120 \times 50 = 300{,}000$ cubic feet, which is thirty times larger than what is usually provided; with complete change of air each hour, one-third of this space will suffice, or ten times as much as is found

in the most liberally equipped school-houses; and if the space be limited to ten thousand cubic feet, the capacity of the standard room, the change should be clearly ten times more rapid. This means that the flow of air should be so abundant as to equal in volume the entire contents of the room in six minutes, effecting a change ten times in an hour. This is only part of the problem, but one which without the complication of other elements, demands some well-studied engineering. Let us suppose that we may neglect both temperature and drafts, and that the air is already at the proper temperature; how can it be moved at such a rate as is shown to be needful? It is important to adhere to this rate, on the one hand not to give less air than good sanitary conditions call for; on the other, not to give more, because that will entail excess of power in the apparatus and excess of fuel to operate it.

Now it is alleged that there is such a thing as natural ventilation. By this expression is meant a ventilation which results without the aid of artifice. Since there can be no motion without force, even natural ventilation must depend upon some form of it. One form of force in natural ventilation is difference of temperature between the air within and the air without the building; but this being extremely variable the force and the effects derived from it must be correspondingly uncertain. Another form is the pressure exerted by the wind; but this not only varies in force, but in direction, and is therefore unreliable in both respects, whose value can be most effectively illustrated by putting it into the shape of a mathematical statement.

If a is a variable quantity indicating the force of the wind, and b another variable representing its direction, then we have x the resultant of their combination, not equal to $a+b$, but to $a \times b$, a quantity which may have any value from nothing to infinity. Now bring the effect of fluctuating temperature into consideration, and the impossibility of obtaining any exact or even approximately satisfactory result will be apparent. There is no choice between these Protean forces. In order to make natural ventilation effective, we must be prepared to utilize either of them wherever found or however fluctuating between the extremes of maximum effect on one hand and almost total restraint on the other, in any of its possible moods, or both of them together in any of the infinite combinations of both their possible moods. This kind of ventilation should be

called accidental ventilation, since by its aid there can be no certainty of effect except by accident, and it is evident that the chance of the accident happening is infinitely remote.

Thus is accounted for and explained the undeviating regularity of failure of all schemes which depend upon accidental conditions for their operation. In fact it should be said that failure is the only element of certainty which they possess.

It would seem as if nothing more need be said to prove the futility of depending upon such uncertain forces as wind and temperature as sources of power to do the work of ventilation. To make the proof overwhelming it is only necessary to show that these forces are not only variable and uncertain, but lacking in quantity. It is extremely seldom that sufficient power can be obtained from them to do this work, which, it would be well to understand, is no small matter. The work of ventilation involves mainly two things to be done which consume power: they are overcoming the inertia of mass and the resistances of friction and change of direction and discharge. I cannot at this time go into any demonstration of the extent of these resistances; it is enough to indicate them by pointing out the weight alone of the air to be moved. In the case of an eight-room school-house the weight of the air supplied for ventilation at the standard rate in one hour would amount to forty-six net tons. Besides overcoming the inertia of this mass in putting it into rapid motion, the sum of the frictional and other resistances acting continuously against its movement may amount to the weight of a column of water one-half inch in height, which is equal to a pressure of about two and a half pounds per square foot. It is usually less than this when the ducts are very large, but even when they are so, the saving by this is more than offset by the devices used to secure thorough diffusion of the fresh air in the rooms if at all efficient. As a rule, the diffusion is very imperfect, causing draughts and local accumulations of foul air which I think can only be remedied by means which will consume additional power. I intend to revert to the importance of uniform distribution later on; I only mention it at this stage because it represents work to be done, and we are considering how extensive this is, how regularly it has to be carried on, and how unfitted for it are the uncertain forces to be obtained from the accidental conditions of wind and weather. It can readily be shown that if the air-ducts are of the usual dimensions and do

not occupy a disproportionate space that sufficient power to move the air can only be obtained from these conditions when the air-inlets happen to face the direction from which a high wind is blowing, or when the external temperature is extremely low. You will observe that when the latter condition exists it is the very time, if ever there is any, when the use of air ought to be economized, for it is then that the consumption of fuel is greatest. It has already been made evident that there can be no systematic ventilation effected by wind power, but since an extremely low temperature may persist for some time, the force derived from it may be temporarily utilized, but it appears that this can only happen when the heating apparatus is loaded up to its full capacity. Consequently natural forces are useful only at times when we are in the worst position to avail of them, and when a liberal supply of fresh air can be least afforded. It ought to be just the other way, because the days of severest cold are proportionately few, and to restrict the ventilation on those days is the only time when, if ever, it ought to be restricted. Moreover, while these accidental forces might be of some aid on the coldest blustering days in winter they are not actually so, because ventilating apparatus is not really designed to utilize them, nor are heating appliances usually sufficiently powerful to stand the strain of heating so much extremely cold air.

To sum up this branch of the subject it is shown that no certainty of effect can be obtained from reliance upon natural conditions, because the forces derived from them are uncertain, irregular, and inadequate, besides which, it is an obvious fact that apparatus cannot be designed to act under such a variety of conditions as must be anticipated without such an excessive complication of construction and management as to overload and obstruct its practical usefulness, to say nothing of its great cost and expense of maintenance and operation. I do not believe the attempt has ever been made to design such an apparatus by any person of scientific training who had by experience acquired practical familiarity with the question. Outside of the tropics natural ventilation exists only as a hypothesis. It is a sort of scientific fetich whose charm has a potency only for the careless and superficial student. So much for the so-called natural ventilation. In these days of systematic and orderly procedure people do not expect regularity of result where the means employed to secure it are accidental and unreliable.

It is now perfectly clear that we can accept this point as settled, and that to assure a regular, quantitative ventilation we must employ some force artificially applied and adapted to the work to be done; that is, it is solely a question of engineering. I have treated this branch of the subject with rather more fulness than it merits, but I cannot neglect the present opportunity to attempt some proselyting with a view to breaking down the superstition which is strangely prevalent in this state regarding the supposed efficacy of natural ventilation, and which I feel it my duty to say seems to be an article of faith of many medical men, even of some who have the reputation of being skilful hygienists.

If, then, artificial force is an essential element of ventilation, what should be its form? This is as much a question of practical economics as of engineering; it depends upon the conditions. The simplest is the heated shaft; one type of it has the heat applied to each extraction-flue separately, or to a chamber in the upper part of a building, to which several flues are connected. This is a convenient method when steam heating is used. But the best type is that in which all the extraction-flues are carried downwards and led to the base of a capacious and lofty aspirating shaft provided with a fire-place at the bottom, a common open grate being as economical and efficient as any source of heat. Sometimes the smoke-pipe of the boilers or furnaces can be made to take the place of the grate; but this should only be done with the advice of a skilful engineer. I know of one hospital where the iron pipe was put in place in such a way that the building was completed before it was found that the extraction-ducts could not be connected to the shaft. I know of another hospital where the iron flue was so chilled by the current of outflowing air which surrounded it in the shaft that the man whose duty it was to keep up the steam pressure, finding he could not do so with a cold chimney, closed the opening into the shaft and thus cut off the whole ventilating movement. The consequences were endured for a long time, but the cause was only revealed after a painstaking investigation, and the difficulty was not an easy one to remedy.

Other forms make use of blowing fans which propel the fresh, or extract the foul, air, or have both in operation at the same time, the power being obtained from various types of motors, in which water, steam, gas, electricity, or compressed air may be the moving

force. Sometimes an aspirating shaft supplements a propelling fan. Still another type utilizes compressed air, delivered in jets in the flues, and so inducing a regular flow. What may actually be the best type for use depends upon the circumstances of the case. The choice must be left to good judgment. As to economy, if fuel were the only consideration, the fan and steam-engine system is preferable, but there are often other considerations which outweigh the question of fuel, such as competent attendance, the form of heating used, etc. For example, where furnaces are used for heat, the appropriate motive power to operate the ventilation is the heated extraction-shaft, although less regular and effective, and as respects fuel, more expensive than the fan. This is the type best adapted to small school-houses, with few rooms, especially in the country districts.

We have seen that the volume of air in ventilation is large, and that force artificially applied is essential to its regular and necessarily rapid movement. There remain to be taken up the questions of temperature and humidity, avoidance of draughts, and thoroughness of distribution. Next to the wind the most uncertain thing is temperature. We can successfully evade the former by refusing to have anything to do with it, but the latter we must be prepared to deal with and bring to a fixed and regular standard no matter where we find it nor how quickly it changes its state. The heating apparatus must be capable of effects as variable as the temperature itself. It is needless to say that this is an embarrassing problem, which as yet has found no solution which is commendable as a simple and practical one for common use. I hazard the opinion that the regulation of temperature in schemes of ventilation, such as that of school-rooms, is the most difficult branch of the subject, yet it is quite ignored or superficially touched upon in all of the treatises with which I am familiar and which investigate almost all the other branches with great thoroughness. Morin, the distinguished, but I must say unpractical, engineer, did not appreciate it at all until after he had published his great work on ventilation, and the failures of apparatus, based on his elegant and abstruse calculations, began to come home to him. By the way, let me throw out the hint that you will find Morin to be a first-rate example of the scientific snob. Admire his book, it is the fashion; but if you want information go to good, honest, unpretentious, and reliable Péclet, who all his life

was kept in the background by the undue influence of Morin's army and social prestige.

There are other people like Morin, perfectly sane on all other subjects, who become victims of an hallucination that they have a call to attack the problem of ventilation. Intelligent men usually recognize the necessity of special study to fit themselves for any line of work; but when ventilation is the question there is no lack of people who will take hold of it as unreflectingly as the greenhorn who, when asked if he could play on the violin, replied: "I guess so; I never tried." Ventilation, as a practical art, has been sadly discredited by educated men who have undertaken it without proper preparation. The chief stumbling-block which overthrows these over-confident venturers is the management of the heating, the regulation of the temperature, which always has been, and still is, the most difficult element of the problem. It is unfortunate that this fact is not appreciated by writers on the subject. I have the best of reasons for knowing that whatever measure of success in actual results has been obtained by "professors" or "doctors" whose title may have been fairly earned by faithful work in their legitimate occupation, but who were lacking experience in this work, has been due in all cases to some silent partner, whom you will find to have been a man who united to a thorough practical knowledge of heating a skill quite analogous to the *main d'œuvre* of the artist, which is acquired only by diligent practice and patient observation.

When a furnace or hot-water apparatus is the source of heat a heating effect may be obtained which is approximated to the demands of temperature. It is found practically, however, that the approximation is not near enough to depend upon, and it is necessary to use the device known as a mixing-valve, which by admitting a greater or less portion of cold air directly to the flue conducting the warm air to the room from the heater gives control over the temperature, providing it is attended to.

This device seems very simple and attractive, especially in a neat lrawing on paper. In fact, it is but a make-shift contrivance, well enough if only one or two of it are used in one building, but applied to every flue it becomes a mechanical nuisance, yet a nuisance which in many systems we cannot get along without for want of something to take its place. In my own work I make considerable sacrifices to avoid using it.

Steam apparatus is ill adapted to variations in temperature, but the facility and economy of the distribution of heat in any direction by its aid, even from remote sources, greatly recommend its use; and since by the aid of the mixing-valve the same result may be obtained as with a furnace or hot water, I think it preferable for large buildings. Some method of automatically controlling the temperature is urgently needed, but nothing has yet been evolved which is cheap and simple in operation. There is, however, an electric contrivance in the market which is in successful use, but which is too complicated and expensive for general application. The need of such a thing has not been felt long enough, nor is it widely enough recognized to attract the attention of inventors. This is mainly because it is only when the air is changed rapidly that it is important to regulate the temperature very closely. Before I leave this question, which I think in its present state is the most unsatisfactory of all, I wish to emphasize the fact that the less such devices are resorted to the better the result will be. If they cannot be avoided it is well to use as few of them as possible, and one way of effecting this is to concentrate the control. If the temperature can be regulated at a central point for the whole building, it is manifestly better than separate mixing-valves for each flue and each room, because it is a way to concentrate and simplify the management, which, it does not need any argument to show, contributes more to successful operation than any other feature. I beg you will weigh this statement carefully. The shortcomings of ventilating apparatus are not always so much owing to omissions in the design as to the neglect of the attendants to make the proper adjustments at the proper time and in the right way, or their inability to care for a vast number of confusing details. The careless and stupid we have always with us, and besides the careless and stupid, the shirking janitor must be included in the design as an unavoidable part of it. Woe to the designing engineer who leaves him out of account! I do not mean that these men are careless or stupid except as regards their duties; in one direction, that of saving themselves trouble, they are assiduous and intelligent in a high degree. Their work, even if arduous, should be so laid out for them that circumstances will exact it and forbid neglect of it in spite of them; they should not be required to do any reflecting or to exercise any judgment, but be furnished with the means of producing required effects with simplicity and certainty and without possibility of evasion.

I must refrain from discussing the question of humidity. It is a moot question, and I think, being the least important part of the subject, we can afford to put it aside until the more essential elements have been definitely settled. Some misapprehensions concerning it, however, are very prevalent, and I think a word or two tending to remove them may not be a waste of time. Natural air contains the vapor of water at all temperatures, but its capacity for absorbing it increases rapidly with the temperature. For example, at thirty-two degrees a cubic foot of it can hold two grains of water, but at seventy degrees it can hold eight grains, though it weighs less because expanded by heat. In nature it is only at times saturated, the mean in this climate being seventy-one per cent of saturation or seventy-one per cent relative humidity, while it varies between the low limit of thirty per cent when it is extremely dry and one hundred per cent when it either rains or snows. If we take air from out of doors at thirty-two degrees and at seventy per cent relative humidity and heat it to seventy degrees without adding water, the 1.4 grain by weight which at first represented seventy per cent, in the new temperature represents only seventeen per cent relative humidity, which is an abnormally low proportion. This is not because the heating process has dried it, as commonly supposed, but because, by the rise of temperature the capacity to absorb water and the avidity for it are enormously increased. Air as dry as this is very disagreeable to many people, but I am inclined to think it is apt to be so when there is a preponderance of foulness along with the dryness. If the air is fresh, it may perhaps be dry without harmfulness; at any rate it is not proved to be hurtful in that condition. On the other hand, if we moisten somewhat the air which we heat, we shall be imitating nature, a safe guide.

The next step to be taken is to provide against the movement of the air being felt by the occupants. The volume to be poured into the room is so large that unless the inlet is judiciously placed and carefully proportioned, drafts will be felt by some of the occupants. The difficulty is increased in proportion as the space is diminished, and is obviated by adding to the number as well as the size of the inlets. The common registers are not suitable for inlets; the openings should be so constructed as to cause the air to flow at a moderate velocity and to reach every part of the room without motion in the vicinity of the occupants greater than two or three feet a second.

The location of the inlets is consequently a matter of prime importance. In the early days of ventilation, their proper location was thought to be susceptible to theoretical demonstration; Morin, heading the theorists, advocated placing them at the top of the room, but the practical men insisted on having them at the bottom. The difficulty with purely theoretical deductions is that they cannot deal with more than one or two elements at a time if they are variable; that is, a certainty cannot be deduced from a variety of conditions unless the number of them is very small, and it is in fact impossible to take all of them into account even if every one is anticipated and recognized. But when you come to a trial, something of importance is almost sure to turn up which was wholly overlooked, yet capable of influencing the result. The first case which was submitted to me for practical solution was Sever Hall of Harvard University in 1878. Accepting the method of practical men in bringing the air in at a low level, I was confronted with the questions of drafts and distribution, to avoid one and insure the other. After balancing together theoretical considerations and such experience as I had personally the advantage to have gained, I concluded to place the inlets as low as possible, which location was in fact high, for it was above the level of the occupants' heads, about half way between floor and ceiling. They were placed on the warm side of the room, and the outlets near them in plan, but at the level of the floor. The arrangement worked well, though not without some complaints of drafts. I used the common registers to cover the openings, which I said above are not well adapted to a real ventilation such as this was, for it was the first educational building to be ventilated by a fan. This arrangement of inlets and outlets was shortly after adopted in the Bridgeport, Ct , High School, and has now come to be the standard method, being used by the two largest firms engaged in school-house ventilation as a specialty, and advocated by experts. I do not consider that the provision is complete for removing the air without an additional outlet at an upper level, which may be opened in case the room gets too warm, and be kept permanently open in summer. To omit it is in the direction of simplicity, which is no small advantage; but I have not yet seen sufficient reason to sacrifice its benefits on that account.

Finally, let us take up the question of distribution. This involves the means of assuring to each occupant his share of the fresh air.

The problem is complicated by the importance of not wasting the heat which has been imparted to the air by the apparatus in the cellar, to be transferred and given up to the room by it before it escapes, for in all types of apparatus, except that which is peculiarly my own, the fresh air is required to do double duty; it is a carrier of heat as well as a diluent and displacer of vitiated air. From this it follows that the entering air must be warmer than the air contained by the room, consequently it rises. Now it is perfectly established that the bodily exhalations, carbonic acid and all, rise also, and that at the top of the room, if occupied by a number of people, the foulest air is to be found. Therefore the first thing that happens to the entering fresh air is its mixture with these exhalations at the upper levels, and the compound has to be cooled by walls and windows before it can descend to the breathing line. Does it not seem desirable that some way should be found to cause this air to be diffused at a low level and follow the exhalations rather than meet them when already on their way out, thus rising through the breathing line before it has required any taint? Moreover, in a sanitary point of view, is it not of paramount importance that the exhalations from each person should be swept directly away altogether if possible, rather than that they should be brought back to another, bearing perhaps some specific contagion? For it must be borne in mind that though dilution by fresh air diminishes the chances of infection by foul air, it is not a perfect safeguard against it. How is it that the school becomes so often a focus of disease of the greatest danger to the health of the community? If an infectious disease breaks out in one home, it can easily be held in check by isolation; but if it breaks out in a school, there is no way to check it short of closing the school and suspending its work. This is not only because it is dreaded as a possible focus of disease, but as a focus whose effect the school-house itself may greatly magnify by its imperfections. These considerations make it evident both that perfection is not yet reached in the matter of the introduction and diffusion of the air, and that improvement in the method would be highly advantageous.

As the result of experience in trials on a large scale I have found that thorough diffusion, or rather a quantitative distribution to each occupant, permits an enormous reduction in the standard amount of air, which it has been observed should not be less than fifty cubic

feet per minute. You will remember how this standard was fixed; it was with the assumption that the fresh air acts as a diluent to keep down the accumulation of exhalations, and that owing to imperfections of apparatus and consequent bad diffusion it is a low requirement, it ought to be somewhat higher. Beginning with thirty cubic feet per person per minute, that is, distributed to each one, and working downwards, I have found that the quantity may be reduced to fifteen cubic feet; that this is ample, and can be supplied readily without producing drafts, a great advantage, more than compensating the reduction in volume, which, however, is not so much a loss as a superfluity, since what is not needful is superfluous. Possibly it may be still further reduced, even reaching the standard of ten feet recommended by Dr. Reid, or the five cubic feet of the early speculators on the subject. This, in my opinion, is the direction in which economy and efficiency should be sought; I mean that the diffusion system, which is suitable for ordinary inhabited rooms and inclosures subject to a variety of uses, should be abandoned in the case of rooms where numbers of people meet, and that the distribution system should be adopted.

We have now been over most of the important ground relating to this subject, but only superficially; as I said at the outset we would have to be content with a brief examination of the leading points, and many of the statements made will have to be taken on trust. Before concluding, it may not be out of place for me to describe here some of my own methods of meeting the difficulties which I have called your attention to, particularly the regulation of temperature and the diffusion of the air. Where steam or hot water is used, it is the feature of my method of treating such buildings as school-houses, that the heating and ventilating are entirely separated. The rooms are all heated by local concealed radiators which have no connection with and no relation to the ventilation. The air is abundantly supplied, but only during the hours of occupation; that is for twenty-five hours a week. Notice what a saving there is in this; using only fresh air at the times when it is needed. It is warmed uniformly to seventy degrees as a standard, never any higher nor any lower, and it is all treated at a central point by efficient and positive but simple means. This reduces the entire management to the lowest terms, and it is obvious that it cannot be surpassed for economy of fuel. As to the temperature in the rooms, variation must be ex-

pected in the case of the local radiators, but the effect of neglecting to watch and regulate them is vastly different from what it is with any other system, because variations in either direction are modified by the constant flood of air which is poured in at seventy degrees, entirely changing the contained air every five or six minutes. This steady flow of air at seventy degrees is the balance-wheel of the system, and prevents local fluctuations quite effectively, even if the radiators are not strictly proportioned in heating power to the exposure, and the steam pressure is not quite suited to the weather for the time being.

As to diffusion, let it be understood that the room and all it contains are warm, thanks to the direct radiators. The temperature is apt to be higher than seventy degrees, influenced in part by them and in part by the occupants. The fresh air from the central apparatus is cooler than the contained air and tends to fall rather than to rise, which is evidently preferable to what was pointed out to be the usual, and, under the conditions, necessary, course. The logical deduction is, that with this system the escape of air should be by way of the upper openings. I am not yet able to state whether this is practically correct in the case of a school-room, but before many months it can be tested exhaustively. I would say that at the new building of the Institute of Technology, for which I designed the system of ventilation in 1883, the upper outlets are never used, but it is important to add that there is no direct radiation, and that the building has no interior finish to protect the cold from penetrating the thin, bare brick walls.

This building is well known as possessing the most thorough ventilation of any in the world; the air never reaches a degree of vitiation which is more than nominal, and the temperature is manageable throughout within close limits, though the construction presents great difficulties in heating. This case is one in which the system of local radiators which I have described would have been most appropriate, but it was the one thing that the Institute authorities objected to, as they preferred what is called indirect radiation, on general principles.

Our review of the progress in school-house ventilation will not be complete without a notice of what is being done in that other department of sanitation, the construction of water-closets and urinals. A new system, that is, new in this locality, has met with great favor,

the dry-closet and urinal system introduced by the Smead Ventilating Company. The credit (and I think it is very great) of introducing this system into school-house construction belongs, I believe, entirely to that Company. There are also the systems of the Fuller and Warren Company and of others who have been quick to appreciate the advantages of the Smead system, and by improving upon it, or by slightly changing the construction, have been able to gather part of the harvest which the latter sowed the seed of. There can be no doubt that the system is efficient and safe, besides being simple to construct and maintain. I regard it as safer than the water-carriage system, because with the latter, careless construction and neglect to detect defects positively insure sickness sooner or later, the drain-pipes and their moist contents favoring fermentation and the generation of noxious gases and disease germs.

With the dry system, the moisture essential to the propagation of these dangerous things is absent, so that the consequences of suspended action or occasional back draft are of trifling importance; and if we must expect imperfections, I should say they would cause vastly less injury to health in the dry than in the water-carriage system. Besides, with the former not only are the consequences of bad work likely to be less dangerous, but the chances of the work being bad are much diminished; and still another advantage is the immunity from frost, disarrangement, stoppage of traps and pipes, and repairs. I believe the system is perfectly safe, practical and commendable, especially where there is no system of sewerage.

On the other hand, it is not probable that the water-carriage system is to be displaced by it. The tendency in all departments of building construction is to elaborate and refine, to increase comforts and conveniences, and not to be governed by practical and economical considerations alone. We are getting farther and farther from simplicity every day, and are adopting complex and costly expedients, provided they offer substantial advantages. We have constant evidence of this on every hand, and it is probable that this question will be controlled by the same tendency, and that while we may install the dry-closet system in particular cases, it will be because it is better than the horrible privy rather than because it is preferable on practical grounds to the water-closet system, which has established itself too strongly to be ever overthrown, as being the highest type for comfort, cleanliness, and convenience. I can-

not leave this topic without saying a word upon the importance of exacting the utmost care and cleanliness in the use of this apparatus on the part of every occupant of the building.

In this state, the descendants of the old colonial settlers are being rapidly driven out and replaced by the children of ignorant and degraded foreigners, coarse in their ideas and filthy in their habits. It is of great importance that these young savages should be taught not only those things which will make them useful as citizens and producers of wealth, but those things upon which the health and comfort of the community depend; and the first step in doing this is to teach them to be personally clean and cleanly in their habits. The teachers should personally supervise the sanitary arrangements, and exact from their pupils the most careful use of them.

Let us not flatter ourselves in the face of the social conditions which seem to be changing for the worse rather than for the better in our state of Massachusetts, that our civilization is assured; this cannot be while ignorance and degradation are so disproportionately large. The process of refining the crude elements of society must go on continuously, for without refinement the sentiment is absent which appreciates the true value of the ounce of prevention, and lends a willing aid to the enforcement of the laws of hygiene. (Applause.)

RABIES,

WITH AN ACCOUNT OF PASTEUR'S WORK AND METHODS.

BY HAROLD C. ERNST, M.D.

Mr. President and Gentlemen: — It is hardly a courteous thing, I fear, for me to be here this afternoon at all, because through some misunderstanding I was unaware that to-day was the day upon which I was expected to speak, until yesterday. Even with this amount of time, under ordinary circumstances, I might have been able to offer something which would be a little more connected than what I have to say. but circumstances with me just now are extraordinary, and in addition to an enormous amount of experimental work that I am endeavoring to conduct properly, I am suffering anxiety in regard to a member of my family. As I told Dr. Durgin yesterday afternoon, from Thursday morning last until Monday night, I have not had my clothes off at all; therefore, I beg your indulgence, if what I have to say is a little disconnected, and if it is not as full as I should like to make it.

In regard to this question of rabies, of course it is entirely unnecessary for me to say that there has been and is a very great diversity of opinion among men, perfectly competent to judge, in regard to the existence even of such a disease. But the advances in our knowledge of pathological processes that have been made within the last few years will enable us, I think, to look at the question from a little clearer point of view than it has been possible to do in the past, and I hope I shall be able to show you that there is distinctly such a disease as rabies, that it is a specific disease, and that it depends upon a special virus which, whilst not isolated as to its identity, has certainly been traced to its place of most common occurrence. Now in considering such a point, the descriptions of the older books are distinctly at fault. The descriptions of the disease that was called rabies are almost diametrically opposite, — if different authors are consulted. The reason for that, I think, is shown by the result

of the experimental work that has been done. One of the main points to be remembered is that it has been distinctly shown that the especial seat of the virus of this disease is in the spinal cord, not in any particular part, but in any portion of it from the lumbar region up to and including the brain. It is perfectly evident that any one portion of the central nervous system being affected, it is the portion of the muscular system governed by that nervous system that is particularly disturbed, and it is the symptoms shown by the muscular system that are especially noticeable to the eye of the ordinary observer; therefore, the clinical symptoms as recorded of rabies, it is easy to conceive, may be anything that may result from the irritation of any portion of the central nervous system, and that is the reason for the descriptions we have of furious rabies, dumb rabies, paralytic rabies. That is a point which, it seems to me, has not been sufficiently made clear, that by all the laboratory experiments that have been carried on, it is distinctly shown that the seat of this disease is in the spinal cord and in the medulla, — in the central nervous system; and therefore any symptoms described in the books may be from a case of true rabies, but there is no one picture to be given of the disease that will enable the ordinary clinical observer to make a proper diagnosis.

Now that there is such a disease, as it seems to me there is, and that it is produced by a virus introduced into the central nervous canal, and possessing a pretty definite period of incubation, with no very definite symptoms excepting any that may result from the irritation of the central nervous system; that this disease may be produced by the inoculation of healthy animals with portions of the cord or brain of diseased animals; that it may be carried on through a long series of years, it seems to me that there is no question at all; that it can be the result of but one form of virus also, it seems to me there can be no question. It is not possible to dispute the assertion that the introduction of various materials under the meninges, as Pasteur has done in carrying on experiments in rabies, may produce similar symptoms to those of rabies in the first animal experimented upon, but it has been shown by no one and at no time that long series of inoculations can be carried on from one animal to another week after week, month after month and year after year, with the production of precisely the same symptoms in every case, excepting in this one instance of the work done in Pasteur's laboratory; and

therefore, it seems proper to me to consider the disease that Pasteur has been handling, and that was derived from the saliva of a child dying, after the bite of a dog, as a specific disease named rabies. It is from that point of view that I spoke in a discussion upon this subject last fall in New York. Dr. Spitzka made the assertion (his remarks are in print, so that it is not too soon to announce them,) Dr. Spitzka made the assertion that he was carrying on in his laboratory experiments with an organism which would produce precisely the same symptoms as are produced by Pasteur's virus; and he also said that this organism carried on through animal after animal would continue to produce the same thing. If that be true, it does not, to my mind, vitiate Pasteur's experiments at all; it merely indicates that we have two viruses that may produce the same clinical symptoms, precisely as it is already shown that we have more than one specific organism which will in medicine produce croupous-pneumonia, and yet many medical men to-day will deny the possibility of there being more than one form of croupous-pneumonia, while it is distinctly proven there are several. Therefore, in the same way we may be on the verge of the discovery of a second virus producing symptoms similar to that of Pasteur; but this is merely a preliminary announcement, and is not yet proven.

But even if it should be proven, it seems to me quite distinctly proper that the term rabies should continue to be applied to this disease that Pasteur has been handling. It was derived by him originally from a child bitten by a dog supposed to be rabid. It has again been derived by him from dogs affected by what is known as street rabies. I have myself obtained it in the same way, this similar disease, in rabbits and guinea-pigs, and many other observers have done the same thing; and that there is this virus to be obtained from animals dying in the way that is supposed to be the way in which animals affected with rabies die; and that it can be made to do all that Pasteur has said that it can be made to do, there is no question, if one can believe the evidence of one's own eyes; and so, again, it seems to me distinctly shown that we have to do with a specific disease, that there is such a disease, and that it does produce all of the symptoms that are spoken of in our text-books; but no text-book yet written has given the proper description of the disease as it may exist, because such a description would include all symptoms that might arise from the irritation of the central nervous system.

Now it has been shown certainly that the virus is present most prominently in the brain, and in the spinal cord to a less degree. It is also present in the organs of the body, in the lymphatic glands, and in the saliva. That has been shown by experimental work in the laboratory, carrying out a similar series of experiments to those that have shown its existence in the spinal cord. The special point which is to be made from this fact, and from what I have already stated in regard to the special position of the virus in the brain and cord, is that no absolute diagnosis, to my mind, of rabies can possibly be made except by the experimental method; that in order to be absolutely certain that one is dealing with a case of rabies, either in man or in the lower animals, it is necessary that the experimental evidence in regard to the disease should be carried out; and certainly the corollary from this is, that before the evidence is obtained, startling the public should be avoided with all the strength which one has at hand.

Another point that can be readily deduced from these facts is that the disease is transferred only by inoculation, that the virus either in the cord or the brain loses its strength after a certain length of time; in other words, if it be a bacterium that produces the disease, that bacterium is distinctly parasitic in nature, and not saphrophytic. It cannot develop outside of the living organism, and therefore this disease can be carried, and carried only, by inoculation, and the inoculation usually occurs, of course, by a bite and introduction of the saliva from a rabid dog.

Then the question arises, naturally, inoculation having occurred, what should be done? The ready method, the one with which everyone should be familiar who runs any danger of coming in contact with rabid dogs, is, if the bite be upon the extremities, upon the finger, upon the hand, upon the arm, or upon the lower extremities, in some way ligation should be practised as speedily as possible, and then very vigorous sucking of the wound. Cauterization, it seems to me, is a relic of the dark ages. I do not believe it does any more good than to ease the patient's mind, on the one hand, and on the other, perhaps, to create a local irritation that may be of service in carrying out the same thing, — the easing of the patient's mind; but beyond that it has not anything to do, and rarely has done any good, and it sometimes produces a very bad ulceration. The same thing, as far as possible, should be practised in the case

of bites upon the body, although in such cases the safety of the patient is much greater than is usually supposed, because in the clothed portion of the body the teeth of the animal inflicting the bite are partially cleaned in passing through the clothing, and the danger of inoculation is greatly diminished. That is the immediate step — ligation, if possible, and sucking of the wound immediately. There is no danger at all in taking this virus into the mouth, if the mouth be in a properly healthy condition, and therefore sucking is the one thing to be depended upon, aided, if possible, by ligation.

Then comes the next point as to the advisability or not of protection by inoculations as suggested by Pasteur; and there has been so much discussion in regard to the effectiveness of his methods, of the way in which he has carried them out, that the controversy has become almost classical, so that it seemed to me it would be of interest to you if I gave simply the details of the various articles in which he introduced the subject of experimental rabies to the world. The first one was published in the "Comptes Rendus de l'Academie des Sciences," in Paris, upon the 24th of January, 1881. The title of it was "On a new malady produced by the saliva of a child dead of rabies." That certainly is an extremely modest beginning. The point that I wish to make in this part of what I have to say, and in all of it, in fact, is to insist upon the perfect frankness and simplicity of Pasteur and his work. He has been accused of all sorts of quackery, and with no justification whatever, so much so that in a discussion in which I took part at one time, the gentleman who led off in the discussion — there were three of them who preceded me — and who meant to give a fair view, I believe, of the subject, was ignorant of the fact of the existence of the magazine called "Annales de l'Institute Pasteur," in which for four years there have been monthly records of every case that he has had; they did not know that that was in existence, and yet accused him of keeping his results secret.

Now the second article published in the "Comptes Rendus" was upon the 11th of December, 1882, nearly two years afterward, and that was merely a continued study of this same disease, the title being, "A continued study of a malady produced by the saliva of a child dead of rabies." On the 25th of February, 1884, he published an article emphasizing the occurrence of the paralytic form of rabies resulting from his experiments with this saliva, this paralytic form

occurring entirely in animals in the laboratory, the so-called furious form being a very rare manifestation. Then on the 19th of May, 1884, he first called attention to the attenuation of the virus, and the possibility of so changing the strength of this virus that instead of producing death, as it had certainly done before, it could be introduced without producing more than very mild symptoms of paralysis, from which the animal would recover.

Then again on the 11th of August, 1884, at the International Medical Congress in London, he emphasized this possibility of the attenuation of the virus, and from it drew a number of interesting conclusions in regard to the possibility of preparing viruses for the prevention of other diseases besides rabies, others that are more common, making this the text for throwing out broad lines of investigation that certainly will bear fruit as the years go on. And upon the 26th of October, in 1885, came the announcement of a method for the prevention of rabies after the bite of a rabid animal. Then on the 1st of March, 1886, and on the 12th of April, 1886, and again on the 1st of February, he published results, the preliminary results of the application of this method to human beings, and ever since that time, as I said a moment ago, there has been a monthly publication and tabulation of every case that has come under his care, so that he who runs may read, if he chooses to look at the tables. And it has been perfectly extraordinary to me that gentlemen who have been interested in this question have not known even of the existence of those tables. One of the great neurologists in this country less than a year ago in speaking of something I was doing said, "Don't follow Pasteur's method, because he doesn't publish his results," and I asked him if he had ever seen these tables. He said he had never heard of them. Now that is not fair. As far as any man can be frank, I think Pasteur has been upon this question, and in discussing his results, it is only fair to look over what he has to say. In these results he claims a lessened mortality, and the figures bear him out; he has certainly reduced the mortality from rabies to less than one per cent, which is very much less than the mortality from such cases, as appears by all the records we have had before. It is impossible to give it exactly, but certainly it was very much larger. Every author who has written on this subject puts it above fifty per cent. Those older statistics were made up when it was impossible to separate the cases where the bite was from an animal truly affected

with rabies from those that were not. They included also those that any passer-by happened to say had rabies, therefore it seems to me if anything is done by the French work, this diminution in mortality is certainly shown by Pasteur's statistics.

Now in regard to the method of attenuation of this virus and its application to human beings. A few words, I think, will make it very plain to you. By a series of experiments it was shown that the cords of animals dead of rabies, if they were removed at once and placed in a dry atmosphere at an even temperature, in fourteen days would lose absolutely their power of producing any symptom whatever of rabies when inoculated, and this loss of virulence was progressive. It went on at the rate of about one-fourteenth on each day, so that at the end of eight days it was about four-sevenths gone, and so on. By experiments upon animals, the introduction of a portion of this cord in an emulsion, a sterilized fluid of a few drops introduced under the skin of an animal of the weakest, and on the following day an emulsion of the cord of the next strongest virus, produced immunity in the animal experimented on, — to an emulsion of the cord from an animal that had died of rabies. And it was because that fact had been well established in the laboratory, and because it had got out, not because of the public announcement of it, that Pasteur was first forced to make his experiments upon human beings. Patients were brought to him, and it was practically impossible for him to avoid trying this experiment upon human beings; and so far as can be seen by a conscientious examination of the statistics, if they be true, which I certainly have not had time to make, but from what I have made within a year, no death can be laid to the door of this method, as has been asserted in a number of quarters, no death at all; and as I have tried to emphasize, if it is possible to believe anything, the mortality from rabies has been distinctly lowered by the introduction of this means of prevention.

One more fact that has been pretty fairly well established by all that has been found in this experimental study is that the period of incubation of rabies is brought somewhere within proper bounds; that in the case of a bite from a rabid animal upon the exposed parts, the face or hands, the danger period is fairly well passed at the end of six weeks. The treatment, if treatment be decided upon in this way of the preventive protective inoculation, should be begun as soon as possible after the bite, but there is practically no danger if it be

delayed for ten days or a fortnight. The cases of so-called rabies resulting seven, eight or ten months after a known bite, are either not true rabies at all, or else they are the result of a fresh inoculation, not the result of a bite that was known; the inoculation probably comes from some source that the patient knows nothing about.

In regard to pathological appearances, a few words may be of interest. They are very few in gross, only the appearance of congestion of the internal organs. The distention of the external vessels of the membranes of the cord and brain is about all that strikes one, and of course this has nothing very peculiar about it, and even under the microscope thus far substantially nothing distinctive has been made out. But in two cases in human beings which came under the observation of Dr. Fitz some ten years ago, and which were very perfectly described by him at that time, the same changes were seen that it has been my fortune to see in two cases occurring in human beings within the last year. They consist, in the first place, of a very marked engorgement of the smaller vessels, of an apparent forcing through the walls of the vessels in certain places, of collections of round cells looking almost like localized hemorrhages, and the occurrence of a few miliary abscesses, in other words, the presentation of the appearance of a bulbitis. The results show there is a certain amount of swelling and infiltration, which would indicate an acute miliary bulbitis, but beyond that there has been nothing. At the same time, these appearances have been seen in cases that have come under observation, so far as I know, in this city and abroad; in animals experimented upon, those appearances are carried out always.

In regard to the occurrence of the disease itself: some five years ago I carried out a series of experiments to see whether Pasteur's assertions were true or not, and I became thoroughly convinced that whatever he had hold of, he certainly had a specific virus. Since then it has been my fortune to have a number of cases of so-called street rabies submitted to me for diagnosis, and I have carried on experiments so as to convince myself that the so-called rabid dog of the streets may be the origin of this poison or virus with which he (Pasteur) is dealing. But at that time I tried very hard to find or to hear of a case of true rabies outside of my own laboratory, and I utterly failed. Since I was a medical student when I knew of these cases of Dr. Fitz of which I have spoken, I have never been brought in contact with a case of true rabies either in man or the lower animals until a

year ago last June. At that time through the kindness of Dr. Peters, a dog which had been under his care was brought to me, and we proved the existence of true rabies in that dog; and from that time until I went away last fall, I cannot say exactly how many cases I have seen, but the number has been pretty large. The largest number was during last spring, beginning at about January and running through to May. I heard of a great many others where the symptoms corresponded precisely with those in the animals which were submitted to me, but those of which I speak were only those that I saw and tested. This limited personal experience indicates what has been the experience drawn from all the statistics that Pasteur has collected; that this disease is distinctly not a disease of summer or hot weather; that its time of greatest intensity is in cold weather, in the winter, and that it diminishes along during the spring months through into August and September, when the curve of its occurrence is at its lowest height; therefore, the common notion of its having anything to do with the sexual organs of the dog is a mistaken one; this reason alone is not sufficient for saying so, but it is with everything else we know in regard to the disease.

I have had the misfortune to see one case of true rabies in man. It is the most dreadful thing that one, I think, can be compelled to watch. After the disease has once started, it is absolutely hopeless. It was a true case, because I proved it by the experimental method. Two other cases came to me, where I proved the diagnosis in the same way, and I know of a fourth. Here are four cases occurring in human beings where the disease was certainly true rabies within the last year. It should be said, however, that for the last six months I know of absolutely no case of rabies, excepting in one suspected case which was brought to me during my absence in Berlin, that I have not yet tested. The disease has been distinctly an epizootic among animals and among dogs during the spring of a year ago. As I told Dr. Durgin yesterday, I am not prepared to say what it is now, for the reason that I have been away for two or three months. I have hardly seen a newspaper. I do not know anything that has been going on in regard to the disease in this locality since last October, but certainly for two or three months before that time it had died out of my observation almost entirely.*

* (NOTE. — Since the meeting, up to date [May 14, 1891], there have come to my knowledge two cases of rabies in man, both occurring at the Massachusetts General Hospital, and four cases in dogs. — *H. C. E.*)

An interesting fact in regard to the occurrence of the disease was communicated to me through Dr. Peters, from a gentleman living in the Berkshires who had some friends coming to stay with him who had been there two or three years before, who went out shooting, and upon their first visit they had found practically no partridges, but upon their second they found a great many, and on inquiring the reason, they were informed that all the foxes had died off of rabies. Now whether that has any connection with what has gone on here the last two years, or two and a half, of course it is impossible to say.

I am extremely sorry that I have not been able to present my thoughts in better shape. (Applause.)

RABIES: ITS PREVALENCE AND SUPPRESSION.

BY AUSTIN PETERS, M.R.C.V.S.

Mr. Chairman and Gentlemen: — I supposed this afternoon that Dr. Lyman, of the Harvard Veterinary School, would first address you on the subject of Rabies, and that he would be followed by Dr. Ernst, but Dr. Lyman being unable to be here, the only person that has spoken on it is Dr. Ernst, and that leaves me second instead of third, as I thought I should be. I expected that Dr. Lyman would speak to you on this disease in its clinical aspects, the symptoms in dogs and other animals, the mode of extension, etc., and I thought Dr. Ernst would speak to you very much as he has, looking at it from a scientific standpoint, and telling you of Pasteur's work, his methods, and his successes, so that there would be little left for me to say; but what I had to say, I thought I would write out, for I can always write my thoughts, and read them, more readily than I can speak off-hand, and I had written this paper, assuming that there were to be two preceding me, and I started by telling the following story: —

My position is, I am afraid, like the embryo statesman who had never made a speech. He was asked to go out of town a little way and speak at a political meeting; at first he did not want to go, as he had never made a speech, but some one told him it was easy enough to speak in public,— all one has to do is to tell a few good stories connected together by a little conversation (much as epithelial cells are connected by a little homogeneous cement substance). Upon learning what an easy and simple matter it was he consented to go; he selected three excellent stories to illustrate what he intended to say, and set out full of courage. Upon arriving at the meeting he learned that there were to be two other speakers beside himself, and that he was to speak last. He felt a little grieved when the first speaker told his best story, but still congratulated himself that he had two pretty good ones left. But when the second speaker told

his next best story his dismay can better be imagined than described, as he only had one miserable anecdote left which fell flat when not supported by the first two, and he retired from the political arena a sadder and a wiser man.

Now, I fear that I am in much the same plight as this young man, and after the eminent gentlemen who have preceded me there is little for me to say except to reiterate and corroborate their remarks. Nevertheless, I am glad to have an opportunity to add my quota to the evidence of the prevalence of rabies the past two years, and can best do so, perhaps, by detailing my own experiences with this disease.

I began practice as a veterinarian in 1885; and never met with a case of rabies in my practice until May 8, 1889, when I was called to see a bull terrier belonging to a client, and made a diagnosis of probable rabies. I did not make a positive diagnosis, because I had not seen a case of rabies around Boston, and therefore was not quite sure.

The dog was securely chained, and I left word that he was on no account to be unloosed, and that I would call and see him the next day. The next day I called again, and found that the owner had shot the dog, as he had become so violent that morning that he became alarmed for fear he would break away.

I carried the carcass of the animal in town to the Harvard Medical School, and Dr. Ernst inoculated two rabbits with an emulsion made from the dog's cerebrum and sterilized veal broth,— the cerebellum and medulla were destroyed by the charge of shot, the owner having shot him in the back of the head and neck, so these could not be used, as is usual. One of the rabbits died that night from the operation; the other showed symptoms of rabies May 22, and died the night of May 26, confirming the diagnosis.

I also learned from the owner of the dog, that two or three weeks previous to his dog's showing symptoms a stray dog appeared in the neighborhood acting strangely. He ran into the yard and bit the dog that was shot, and then ran out and bit — or snapped at — two dogs owned by neighbors; the waif then ran off in a southerly direction. How many other dogs he bit in his course, nobody knows.

I reported the matter to the Boston Board of Health when the dog was shot, as the law requires, and my duty too, I thought, and also wrote that I would report the result of the inoculations on the rabbits, which I believe I did.

Since then I have had in my practice nine cases of rabies, all in dogs; — three more in 1889, five in 1890, and one so far this year. Most of these cases occurred in my immediate neighborhood at Jamaica Plain, but I have seen one each in Brookline, Newton, Milton, and Pembroke. The one at Pembroke I think was bitten at Jamaica Plain before being taken away by his owner for the summer.

I have also had one or two dogs killed that were bitten by rabid ones, and have known of a good many cases coming under the notice of other veterinarians, but have reported no more cases to Boards of Health, as it did not seem to me that the one I reported was sufficiently appreciated.

Rabies, like all epizootic diseases, is at times very prevalent, and then after a varying duration becomes very much less frequent, and finally is seldom or never heard of until a fresh outbreak occurs.

When prevalent among dogs, cases are constantly occurring in the human, and as the malady subsides among the canines, fewer or no cases of death from this cause are reported in man.

From Jan. 1, 1842, the beginning of a systematic registration of human deaths in Massachusetts, until Jan. 1, 1890, there were eighty-eight deaths of people with rabies in this state, and the varying number in the last few years make an interesting study.

In 1876 there were four deaths from this cause, then fourteen in 1877; fifteen in 1878, then it gradually subsides, there being five in 1879, three in 1880, three in 1881, and then none until 1888, when there were two; then it increases rapidly, there being fourteen in 1889. The vital statistics for 1890 are not made up yet, but it is likely there will be more cases reported than in 1889, as probably there have been more cases in dogs. Previous to 1876 there were no cases in humans after 1870, when two deaths occurred.

The cases of human rabies occurring in 1889 were by months as follows: —

January	1
April	2
July	1
August	3
October	3
November	3
December	1
	14

It may be seen that it occurs in cool as well as in hot weather, and it also increased very greatly in the last half of the year.

By counties the following is the arrangement: —

Bristol	3
Essex	6
Middlesex	1
Norfolk	2
Plymouth	1
Suffolk	1
	14

For 1890 I think that instead of being confined to the eastern end of the state it will have extended to the central and western portions as well.

There were twelve persons from Massachusetts, who were bitten by rabid dogs, inoculated at the Institute Pasteur in New York City, between Feb. 18, 1890, when it was first opened, and Oct. 15, besides 118 others from different parts of the country, — sixty-four of these coming from New York State. If the present outbreak acts as the previous one, there should be fewer cases this year, gradually decreasing for three or four years, until a period arrives when here are none for several years, when a fresh outbreak may occur.

It is an excellent idea for the Cattle Commissioners to have at last called the attention of local Boards of Health to the prevalence of rabies, and it is to be hoped that the result of the discussion here to-day may lead to active and concerted measures being taken for its abatement.

If action is to be taken for the suppression of this terrible disorder, only the local authorities can properly and thoroughly enforce the law, but they must co-operate with each other.

For example, last summer the town of Framingham ordered that all dogs within its limits must be muzzled. Now the town of Brookline orders all dogs muzzled or chained for sixty days, while most of our cities and towns take no action whatever in regard to the matter; but it is very doubtful if such erratic and independent action has any marked influence upon the prevalence of the disease.

In order to obtain the best results every city and town in the commonwealth should act together; an order should go into effect on the

same day in every township and municipality that every dog found at large, without being properly muzzled, would be instantly shot, whether accompanied by his owner or otherwise; and the order should be thoroughly carried out by the authorities. At the expiration of sixty or ninety days this regulation should go out of effect in every community upon the same day, and should not cease in one until it ceased in all.

If such a step had been taken two years ago this spring when it was at first apparent that we were about to have an epizootic of rabies, thirty or forty human lives might have been saved to the State of Massachusetts. If proper measures are taken at once all over the state many more lives can be saved and this outbreak of rabies brought to a speedier termination. Even then there would be danger of outside infection again unless contiguous states could arrange to act at the same time that Massachusetts did.

Rabies can be spread very widely by an affected dog, and it is not infrequent for a rabid canine to run forty miles, biting many others in his path; it would spread to a much greater extent if it were not for the fact that a very large proportion of dogs attacked with the malady are not seized with the desire to run off and bite all obstacles in their way, but are noticed to be acting peculiarly by their owners, and are secured, and finally are killed or die without doing any damage.

Most of the contagious animal diseases with which our Cattle Commission now has to deal are of as great interest to the sanitarian as to the breeder or owner of live-stock, because so many are common to both man and animals; and it has been a question in my mind lately whether it would not be better to transfer the powers now vested in the Cattle Commission, to the State Board of Health. This idea occurred to me with particular force after reading that portion of the Governor's message in which he refers to the great number of commissions we have, and asks if something cannot be done to simplify matters.

I say this in a perfectly friendly spirit to the Cattle Commission, recognizing the good work they have done in the past, but I merely offer the suggestion, would it not be better to merge matters pertaining to the public health in one bureau, this board not only to do what the present Cattle Commission does, but also to act in a broader scope, considering contagious animal diseases in their relation to the public health, as well as a menace to our live-stock interests.

In such a case the State Board of Health should be made up of the best experts on human and animal diseases and sanitary engineering, to act as an advisory body to local Boards of Health, — the latter to enforce and carry out the laws. The State Board might also require power to stimulate and direct local boards when they were inefficient or lethargic. In addition I believe it would be an excellent plan to have a National Board of Health to secure co-operation between the State Boards of Health.

Because animals have and transmit to man such fatal and loathsome disorders as rabies, glanders and tuberculosis, it does not follow that they are less important than leprosy and typhoid fever, or that it detracts from a man's dignity, knowledge or usefulness, to do what he can to prevent the ravages of the former as well as the latter.

Before we can ever have a system of protection to the public health approaching perfection, it will be necessary to place the contagious and infectious diseases of animals in the same category with those of man, and have the same authorities exercise a supervision over both.

I believe it would be a matter of great utility if veterinarians (that is, qualified veterinarians) were required to make returns of the contagious diseases that occur in their practice among animals. These returns could be tabulated, and although they would not be complete, yet they would show to some extent the relative frequency of the same diseases among man and animals in certain localities, and for this reason would prove of great interest and value.

The law requiring all persons having knowledge of or suspecting any contagious animal disease to report the same to local Boards of Health is a good one and should be lived up to, although I am sorry to say I rarely practice what I preach; for if I can carry out the proper regulations alone I do not report the case, as the owner is generally averse to having it done. I therefore obey only the spirit, but not the letter of the law. But I think that this regulation should be enforced, and that an attempt should be made to tabulate the contagious animal diseases in the same manner contagious human diseases are tabulated, and then valuable tables of comparison could be drawn.

When legislative hearings are to be granted upon any subject of sanitary importance, experts in human or animal diseases, as the case

may be, should be called upon for an opinion, and their evidence should be given the consideration it deserves. Because officers of anti-cruelty societies and professional dog fanciers do not believe in the existence of rabies, or think that muzzling dogs is cruel, it is no reason that such a disease does not exist; and what they have to say on the subject should be taken for just what it is worth,— that is, nothing.

If a man is going to law he has to employ a lawyer; if he has a piece of land to be surveyed he employs a civil engineer; or if he wants a piece of electrical engineering performed he engages an electrician; yet when it comes to questions that more nearly affect our lives and health, then the most foolish and ignorant presume to advance their opinions, and they are accepted as though their mouths were the openings of the profoundest oracles.

A dog should be a luxury and not a necessity, and I am in favor of any measures which will tend towards a limitation of their number and the responsibility of owners, and lead to the destruction of a horde of valueless mongrels and curs belonging to careless and irresponsible individuals. There is too much puerile sentimentality about the "poor man's dog;" and he is too cruel towards sheep, poultry, and other animals to command a great deal of compassion when we consider the property he destroys and the ravages of rabies.

I commenced this discourse without a text; permit me to conclude by announcing that I might have preached from either of the following scriptural passages: —

Psalms XXII.: 20. "Deliver my soul from the sword; my darling from the power of the dog."

Or

St. Paul's Epistle to the Philippians, III.: 2. "Beware of dogs."

I give you your choice of either or both. (Applause.)

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